Table. Haplotype Frequencies Estimated by 3 Programs in Chinese Controls and Cases

	Haplotype	MLOCUS*	SNPHAP†	PHASE‡	Mean (SD)	Coefficient of Variation
Controls	21111211	0.4601	0.4629	0.4629	0.462 (0.0016)	0.0035
	11121122	0.3685	0.3632	0.363	0.3649 (0.0031)	0.0086
	22211121	0.0723	0.0759	0.075	0.0744 (0.0019)	0.0253
	21112211	0.0412	0.0429	0.0427	0.0423 (0.0001)	0.0222
	11121211	0.0255	0.0239	0.0241	0.0245 (0.0009)	0.0356
	11111211	0.0086	0.0083	0.00827	0.0084 (0.0002)	0.0229
	21121122	0.0052	0.0048	0.00484	0.005 (0.0002)	0.0423
	12221122	0.0052	0.0049	0.00483	0.005 (0.0002)	0.03
Cases	21111211	0.3466	0.3393	0.3411	0.3423 (0.0038)	0.0111
	11121122	0.3263	0.3146	0.3083	0.3164 (0.009)	0.0288
	11111211	0.1015	0.1016	0.096	0.0997 (0.0032)	0.032
	11111212	0.0472	0.0438	0.0381	0.043 (0.0046)	0.1069
	21121121	0.0428	0.0407	0.0328	0.0388 (0.00527)	0.1361
	22211121	0.0342	0.0409	0.0343	0.0365 (0.0038)	0.1052
	21112211	0.0211	0.021	0.0191	0.0204 (0.0011)	0.0553
	11121121	0.0195	0.0209	0.027	0.0225 (0.004)	0.1785
	21111212	0.0158	0.0141	0.0167	0.0156 (0.0013)	0.084
	12211121	0.0082	0.0085	0.0068	0.0078 (0.0009)	0.1178

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8 markers from the 2 other similar haplotypes. In our article, we combined haplotypes following the approach that combinations should only be made of haplotypes that are evolutionarily or functionally related. Ultimately, haplotype linkages have to be validated at the level of identification of functional loci and not by P *values, which, as rightly pointed out, may be inflated. Clearly, statistical methods for haplotypes are constantly being improved, and as a result, progressively more accurate* P *values will be generated, leading to linkages that are more likely to be validated.*

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Distinguishing Current From Remitted Posttraumatic Stress Disorder

The article by Breslau et al[1] "Sleep in Lifetime Posttraumatic Stress Disorder: A Community-Based Polysomnographic Study" makes an important contribution to the posttraumatic stress disorder (PTSD) sleep literature, but we are concerned that it may be misconstrued as a study of patients who currently have this condition. Though the term *lifetime PTSD* is strictly accurate, only a small number of subjects (18%) were symptomatic when studied in the laboratory. Nevertheless, long before the sample is precisely described in the "Results" section, the "lifetime" descriptor is dropped in favor of "PTSD" or "the PTSD group," labels understood by most to indicate current PTSD status. Insofar as 82% of subjects were in remission, that studies of remitted patients are rare, and that many of the findings of the study are wholly compatible with remitted status, it seems to us that additional efforts were warranted to establish and sustain in the reader's mind an accurate picture of the sample.

Breslau et al argue that the sleep continuity findings of the 12 subjects with current PTSD were not statistically different from those of the 59 subjects with remitted PTSD, justifying their combination into a single "lifetime PTSD" group. Though adequate statistical power was highlighted for other comparisons, power was not reported for this comparison. Adopting the preconditions of Breslau et al (effect size, approximately 0.35; $\alpha = .05$, 2-tailed; and unbalanced sample sizes of 12 and 59) and using the Cohen estimate for power of a *t* test,[2(p36)] a priori power for comparisons between subjects with current and remitted PTSD was approximately 20%. In this light, little significance can be attached to the absence of statistical

differences between subjects with current and remitted PTSD, or even the directionalities of these differences. The exclusion of medicated subjects probably biased the 12 subjects with current PTSD toward lower levels of severity, while severity was unreported. In fact, the lack of statistical power combined with a presumable bias toward lower severity would have been ample justification for collapsing subjects with current and remitted PTSD. Reporting an absence of differences between these groups without acknowledging lack of power may have led some readers to infer, invalidly, that because subjects with current PTSD did not appear to differ from those with remitted PTSD, and those with remitted PTSD did not differ from controls, then subjects with current PTSD did not differ from controls. Unfortunately, the terminological conventions adopted by Breslau et al are also compatible with this implicit chain of inference, flawed as it is.

If this study is understood as contrasting subjects with remitted PTSD and controls, the absence of "clinically relevant sleep disturbances" is expected, and the persistence of excess arousals from rapid eye movement (REM) sleep acquires new significance. Inspection of the means suggests that had the few subjects with PTSD been excluded in favor of a pure remitted group, the pattern of excess arousals from REM sleep would have remained. This is an exciting finding, particularly in light of the care with which this sample was acquired and studied. Interruption of REM sleep by brief arousals is compatible with recent findings in acute PTSD[3] and with recent animal studies examining the effects of conditioned fear on sleep.[4] It is also consistent with the long-standing interest of PTSD sleep researchers in REM sleep mechanisms.[5] Because 3 studies have reported elevated REM sleep phasic event frequencies in current PTSD,[6-8] further analysis of REM sleep phasic phenomena in these subjects with remitted PTSD may be warranted. The persistence of pathognomonic features of sleep in remitted psychiatric patients has long intrigued researchers interested in pathophysiology and in vulnerability to relapse. The findings of this study represent a novel, important, and welcome contribution to the PTSD sleep literature in this regard.

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In reply

In our article entitled "Sleep in Lifetime Posttraumatic Stress Disorder: A Community-Based Polysomnographic Study,"[1] we report polysomnographic findings from a large study nested in a well-described, longitudinal epidemiologic sample. The PTSD subset comprises lifetime cases, both current and past, combined based on the absence of any evidence suggesting group differences. We found no evidence for clinically relevant sleep disturbances in lifetime PTSD but higher rates of arousal from REM, a finding of uncertain clinical relevance. Clearly, our findings do not lead us to deny the possibility that some PTSD cases have sleep disturbances and could benefit from treatment.

Drs Woodward, Neylan, Mellman, and Ross find in our article confirmation for their beliefs about sleep phenomena in PTSD. They do so by redefining the article as a study of remitted (past) PTSD and discounting current cases as irrelevant for the reported results, which are based on the combined group. Redefined as a study of remitted cases, the results no longer challenge the belief that PTSD is associated with objectively measured, clinically relevant sleep disturbances because (according to Woodward and colleagues) the association applies only to current cases. The redefinition has another consequence. Our findings of higher rates of arousal from REM gain heightened significance as novel; they extend to remitted cases what Woodward and colleagues believe to be a core finding in PTSD.

Woodward and colleagues support their position by noting (1) the small number of current cases in our sample (n=12) and the implication for ruling out differences between current and past PTSD (we address this point later) and (2) the likelihood of a bias toward lower severity in current cases because of "exclusion of medicated subjects." Woodward and colleagues missed our explicit statement that no exclusions based on psychotropic medication use were necessary in this general population sample (see "Sample and Procedures" under the "Methods" section on page 509[1]).

To interpret our REM-specific findings as applying to past PTSD, Woodward and colleagues concede that there are no differences between current and past PTSD. (Group differences would open the possibility that the means of lifetime cases in the comparison with noncases are different from the means of past cases.) In contrast, with respect to our negative findings on clinically relevant sleep disturbances in lifetime PTSD, they reject the conclusion of "no differences" between current and past PTSD. The grounds for the apparent inconsistency are unclear except that acknowledging equivalency between current and past PTSD in the REM findings does not challenge Woodward and colleagues' beliefs, whereas it would do so in relation to the negative findings on sleep disturbances. Surprisingly, the issue of equivalency or nonequivalency appears to be immaterial. Under both conditions, Woodward and colleagues propose that current cases should be ignored and that the findings are about past PTSD.